

In Memoriam

Remembering Nat Schoenfeld

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William Nathan Schoenfeld was born in New York City on December 6, 1915, and died in Sun City West, Arizona, on August 3, 1996. He was an undergraduate at the College of the City of New York, where he received a BS degree in 1937. He received his PhD from Columbia University in 1942 and then continued there as a faculty member, advancing from lecturer to full professor. In 1966 he moved to Queens College of the City University of New York, where he remained until his retirement in 1983. During his years at Queens, he also took on visiting appointments in Brazil, Mexico, and Venezuela. In retirement, he spent roughly a decade in Israel, where he occasionally taught as a visiting professor at the Hebrew University of Jerusalem. He returned to the United States for his final years. He is survived by his wife, Melanie, their three children, Rivka, Joshua and Naomi, and a son, Mark, from a previous marriage.

Nat Schoenfeld is probably best known in behavior analysis for the undergraduate program at Columbia that he and Fred Keller created, for *Principles of Psychology* (K&S; 1950), the book he coauthored with Fred Keller, and for the research that he generated and fostered with his graduate students at Columbia and at Queens. He wrote prolifically on perception, autonomic conditioning, learning theories, verbal behavior, contingencies, the classification of reinforcement schedules, and, most recently, religion and human behavior (1993). He played an active role on the editorial boards of various journals and especially in the founding of the *Journal of the Experimental Analysis of Behavior*; he served as President of the Eastern Psychological Association, the Pavlovian Society of North America, and Division 25 of the American Psychological Association, and he chaired the Experimental Psychology Study Section of the National Institutes of Health, to mention a few

of his distinguished professional contributions.

It is probably less well known that he began his career as a social psychologist; the term *ego-involvement* may have originated in his first published paper (Klein & Schoenfeld, 1941). But Nat's introduction by Fred Keller to *The Behavior of Organisms* (Skinner, 1938) had profound and lasting intellectual impact, and Nat's subsequent contributions link him inextricably to Fred Keller and to Fred Skinner. Together, those three were the founders of the field that we now call behavior analysis.

I only began to see Nat Schoenfeld in these terms, however, long after he had been my teacher, and this is my opportunity to write about some of the things about him that won't be found in his curriculum vitae or in his writings. Fortunately, many details of his professional life and work are provided in an appreciation by Eliot Hearst (1997), and that allows me the luxury of concentrating on what mattered most to me. The trouble with trying to write a reminiscence about someone who has been important in your life, of course, is that it too easily becomes a piece about yourself instead of a piece about the other person. I hope what I

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have to say about Nat Schoenfeld justifies that risk.

I grew up in New York City. Even after I became an undergraduate student in Columbia College I continued to live at home with my parents in the Washington Heights section of upper Manhattan. I worked summers and during the winter break, but with some scholarship help I was able to make ends meet without having to work while classes were in session. In my sophomore year I took Fred Keller's introductory course, Psychology 1-2.

The students in the course came to know of Nat mainly as the coauthor of K&S and as an occasional presence in the laboratory. One story that circulated among us contributed to his formidable reputation. It concerned the role he was said to have played in a class simulation of lie detection in an earlier semester. Two or three students were called out of the laboratory to serve as subjects while the rest of the class was given a briefing. A teaching assistant asked one of them to make a phone call for him from a faculty office, because the class schedule prevented him from making the call himself. The office was Schoenfeld's, and Schoenfeld, who had been waiting nearby, entered his office on the teaching assistant's cue just as the student had started the call. Berating the student for using a faculty member's phone without permission, Nat took over the phone, redialed, and was apparently in the midst of a conversation with the Dean's Office about academic suspension and other disciplinary action when the teaching assistant entered, said the student was needed for the laboratory, and whisked him away. That student and the others were then hooked up to a galvanic-skin-response meter and each was asked to free associate to a word list that included *office*, *telephone*, and *dean* as some of the critical words nested among the neutral ones. Needless to say, the class was able to identify the "guilty" student, but the story goes that, given the quality of Nat's

performance, they really didn't need the physiological measure to do so.

The story did not deter me, and my experience in Psychology 1-2 led me to enroll in Schoenfeld's course in experimental psychology (Psychology 3) in the fall semester of my junior year. Though not called by that name, Psychology 3 was effectively a course in sensation and perception; its text was the 1938 edition of Woodworth's *Experimental Psychology*. Its spring continuation, Psychology 4, was effectively a course in learning and motivation; its two texts were Skinner's *The Behavior of Organisms* and Hull's *Principles of Psychology*. The class met every Tuesday and Thursday morning, with the laboratory scheduled later each day. Eliot Hearst was Nat's teaching assistant during that academic year.

A little before the start of my junior year, my father fulfilled a lifelong ambition: He bought a car (traffic, parking, and the costs of car ownership were less of a problem in the New York City of the mid-1950s than they would be later). He began dropping me off on campus on his way to work to save me the expense of subway fare (and probably to have time to talk too, because he also worked nights and weekends in those days). As a result, I usually arrived at the Psychology 3 classroom in the annex of Schermerhorn Hall an hour or so before the scheduled 9:00 a.m. start of classes.

At Columbia, Nat Schoenfeld routinely began his working day early. Sometime during the first week or so of classes, I had just entered the classroom and was probably reviewing the syllabus or flipping through the textbook when Nat looked in and saw me sitting there. He continued on to his office a little further down the hall, came back a few minutes later and sat down across from me, and we began talking. From then on we did that on most class days throughout that academic year. The class was small: probably not more than a dozen students, all male. As class time approached the other students began to appear and to join in the

conversation. I don't know whether any of them had any idea how often or how long Nat and I had been talking each morning before their arrival.

The topics we discussed were usually unpredictable, and I don't recall any systematic differences between what Nat and I talked about before the others arrived and what we all discussed together. Once when just he and I were talking, our conversation turned to whether I was interested in going on in psychology, and he suggested that I shouldn't do it if it interested me because I thought it might help me to solve my own problems. The possibility hadn't occurred to me, and it took me a while to understand the comment and why he had made it. I'd read some Freud before college and my summer job at the time was as an attendant on a psychiatric ward, but the psychology I was learning at Columbia seemed remote from such issues (I was to learn more about the connections in Ralph Hefferline's course on behavior pathology). I now believe that my reaction appealed to him because it implied that I was interested in the science of behavior for its own sake and not because it was a way-station leading somewhere else.

Mostly, both before and after the other students arrived, we talked about behavior. Sometimes the topic was obviously relevant to the course; at other times its relevance became obvious only a long time afterward, if it became obvious at all. Sometimes the talk was structured around something we'd read or something we'd be doing in the laboratory, and sometimes Nat would pick up on something that someone said and follow up on it in a digression that would continue for the remainder of that class. It's impossible for me to say how many times I plagiarized his examples when I later went on to do my own teaching.

We considered the arbitrariness of the concept of a stimulus threshold in the context of discussions about physiological limits in sports and whether, given a sufficiently large payoff, you

could get reaction times shorter than any that had ever been seen before; as some of us would realize long afterward, the work that would be formalized as signal detection theory over the next decade or so was just beginning to appear in the literature at the time. The breaking and rebreaking of world records across a wide range of athletic events over subsequent years would come as no surprise to us.

We considered the priorities of physiology and behavior in the context of discussions of rods and cones and dark adaptation functions and visual acuity in the retinal periphery; those who followed the argument were persuaded that if you didn't have the behavioral data first, you couldn't tell a physiologist what to look for in the visual system. In these times, as neuroscientists use PET scans and other methods to look at actual events in the nervous system, it is worthwhile to be reminded that behavior still has priority, in the sense that it is the guide to what neuroscientists must look for in the brain.

We considered the contributions of learning and instinct to behavior in the context of discussions about whether a boy and girl stranded on a deserted island as children would ever discover sex; whatever our initial guesses, by the time we had reviewed what was known about the related question of whether a feral child would ever learn to talk, we had begun to understand how little we knew and how much we took for granted. His questions were good preparation for looking at the conflicting claims about language acquisition in children by those who have argued from data and those who have argued from principle.

We considered the role of the nervous system in behavior in the context of discussions of the relative impact on someone's personality of cutting off the tip of the person's nose or removing an equal volume of the cerebral cortex; it should be no surprise that the example in that all-male class was an attractive female. The priority of the nose followed from the fact that the

loss of an equal volume of cortex would probably have no detectable effect on behavior, and we were forced to concede that we had never seen a brain doing anything. But in worrying about whether muscles or other organs (including noses) played a role in learning, we began to appreciate the significance of thinking about behavior as an interaction between a whole organism and its environment.

We considered the meaning of hunger in the context of discussions of a hypothetical caveman who caught and caged a small animal and then began to observe its eating habits; in its treatment of hypothetical constructs and invented causes this deconstruction of reified concepts was an exorcism of sorts. Anyone who later read the opening paragraphs of Skinner's 1932 paper called "Drive and Reflex Strength" would also discover that it was a preparation for the analysis of verbal behavior and the turning of that analysis on the behavior of the scientist.

We considered the nature of science in the context of asking how we could deal with an independent variable as such if it was changed by its own dependent variable, as when the scheduled delivery of reinforcers was affected by the very behavior it was arranged for. It seemed at the time that Nat had a blind spot about interaction, because such interaction seemed inseparable from the concept of reinforcement itself. If the only proper independent variable in science is one that is isolated from its dependent variables, then what do we say about reinforcers when they are produced by the very behavior that we want to measure as our dependent variable?

I was only peripherally familiar with Nat's research at the time, but his system of schedule classification was built on methods for getting around that problem. It's too bad computers weren't available then. For example, the problem that obtained reinforcers deviate from scheduled reinforcers can now be finessed by allowing reinforcement setups to accumulate so that the

organism can produce several reinforcers in a row; the equipment available at the time made such procedures technically unmanageable even when they were feasible in principle. Nat brought the issue up from time to time even in my correspondence with him very near the end of his life.

Nat seemed always to be asking questions of the students in that class, and often when one of us answered he would follow up by asking how we knew. As he uncovered the further assumptions behind those assumptions we'd already had to acknowledge, some students found him exasperating or even intimidating. Those who thought that his point was to illustrate styles of argument or to teach us how to succeed in debate were seriously mistaken. In fact, the surest way to get his disapproval was to give an answer that seemed designed more to satisfy him or to demonstrate argumentative skill than to get at the point at issue. He cared about those questions, and even if he often managed them so that we felt we were doing most of the work for him, it was clear that nothing mattered more to him in that classroom than to have made a little progress toward the answers.

He obviously enjoyed his work, and even his jokes were on target. There was the one about the distinguished professor who was given a chauffeur to escort him to colloquia. The chauffeur sat in on the lectures and after a few told the professor that he thought he could give them too. So they changed places before the colloquium at a university where the professor wasn't known by sight, and the professor, in the chauffeur's uniform, sat in the front row. The chauffeur talked a little faster than the professor, however, so on this occasion there was time for questions. Someone in the audience offered one with the remark that it would be hard to answer. But the chauffeur just shrugged and waved toward the professor, saying, "Your question's not that difficult. In fact, it's so easy that even my chauffeur can answer it!"

Then there was the one about the opera singer who had just finished singing an encore. One person in the balcony stood up and shouted "Bis! Bis! Bis!," a traditional request for a repeat. So the singer sang the number again, and again came the shout of "Bis!" After several repetitions the singer called up to the person in the balcony and asked how long this would go on. Back came the reply, "Until you get it right!" What could be more appropriate than jokes about asking and answering questions and about getting it right?

Toward the end of the spring semester, Eliot Hearst talked to me about the possibility of becoming Nat's teaching assistant the following year. Eliot was leaving Columbia that summer, and Nat preferred teaching assistants who were familiar with the undergraduate program and his teaching style. Apparently none of the available graduate students qualified. If my continuing in psychology wasn't inevitable already, that offer clinched it. The following year, during my assistantship, Nat taught me a range of laboratory and teaching skills from the calibration of a Macbeth illuminometer and the programming of operant equipment to preparation of course handouts and the grading of papers.

At the time, the Columbia department included several chess masters among its graduate students. Nat liked chess but was not in their class, nor was I, so we began to play chess together at lunchtime. Mostly we talked, however, often across the chessboard. He cautioned me about getting into debates with philosophers, and he told me about the difficulties of writing clearly about behavior, and he speculated on the nature of time. In the spring of that year, I also took a seminar Nat taught with Fred Keller and Ralph Hefferline. We started with Skinner's William James lectures, and finished them in time to move on to Skinner's book, *Verbal Behavior*, when it was published later that semester. A lot of our talk, therefore, was about

verbal behavior and its implications for knowing.

Nat moved from Columbia College to the Columbia General Studies program and later to Queens College after that semester. I stayed on for a Masters year at Columbia and then moved on too. Thereafter (but not often enough) we saw each other from time to time in our various travels, usually at professional meetings; later we occasionally talked on the phone and exchanged letters. I had known of Nat's abiding interest in the interbehaviorism of J. R. Kantor and also of his ongoing revision of his theoretical ideas about avoidance behavior. During my editorship of the *Journal of the Experimental Analysis of Behavior*, I had the opportunity to invite him to publish those works, the former as a retrospective book review (Schoenfeld, 1969b), and the latter as a contribution toward a Skinner Festschrift (Schoenfeld, 1969a). Neither article included a formal reference list, and I worried about what precedents they might thereby set. But I accepted them both anyway (later I was wisely advised that anything that seemed to set a precedent could later simply be relabeled as a mistake).

When I wrote a chapter on timing for Nat's edited book, *The Theory of Reinforcement Schedules* (Schoenfeld, 1970), I was surprised that he made no editorial changes. I was bothered by one of my sentences once the chapter was in print, and some time afterward I mentioned it to him when I saw him at a meeting. He remembered which sentence it was and agreed with my uneasiness about it. He then launched into a discussion of the short-term and long-term consequences of editorial practices when editors change what authors say instead of letting authors speak for themselves. In the context of that discussion and others on different occasions, I realized that it was probably a good thing that I hadn't asked him to add formal references to those two articles I had invited; if I had insisted he probably would have withdrawn them. I had to remind myself

that for Nat references were appeals to authority rather than aids to the reader. In any case, it is better for us to have those articles without formal references than not to have them at all.

One way to judge people is to ask about the reinforcers that maintain their behavior. By watching what people do, we can often judge what is most important to them. The reinforcers might include recognition and acclaim or social consequences or material things or sexual conquests or political power, among the many possibilities. For Nat, it was figuring out things about behavior, and behavior included all of life. That's why he wrote about verbal behavior and religion as well as about reinforcers and contingencies, and that's why he argued that one shouldn't go into psychology to solve one's own problems. He was so adamant about keeping the personal dimension out of our behavioral inquiries that my one reservation in writing this reminiscence is that had he known about it he might not have approved. And probably that's why the inquisitiveness and integrity of his questions meant so much to so many of us who were lucky enough to have had him as a teacher.

In one of the last few phone conversations I had with him during his last year, he talked about three projects he would like to have finished if he'd had the time: a poem, a philosophical piece

on a small point that would be perplexing to philosophers, and a translation of Genesis for physicists (e.g., the world without form and void as a world of maximum entropy). But what moved me most of all was when he talked about the fun we would have had if we could have gotten together to talk more often. Talk for Nat was never competition, nor was it talk just for its own sake. And what fun we did have. His talk made all the difference in the world to me, and I'll miss it, and him, more than I can say.

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